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ABSTRACT

The basic premise explored in this paper is that the conception of educational research which has dominated thinking in the past and continues to influence how the research community thinks and what it does today is inadequate and to some extent, inappropriate. This paper is divided into four sections. Section 1 explicates the traditional conception of research in education and discusses the role research was traditionally expected to play in education. Section 2 focuses on more contemporary views about the form and function of research in education. Evidence of both growing skepticism about traditional views and continued acceptance of traditional notions is presented. The third section focuses on problems with the traditional views of research and the traditional view of research's role in applied public policy fields such as education. The final part of the paper focuses on implications of the two problems discussed in Section 3 for rethinking the form and function of educational a research in general and for constructing a research agenda for the National Center for Science Teaching and Learning. (PR)

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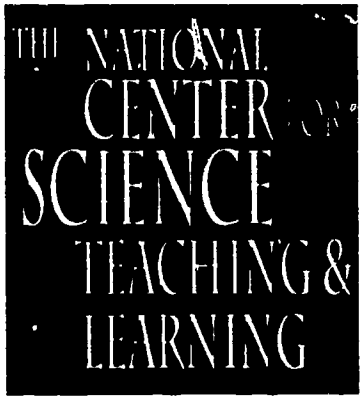

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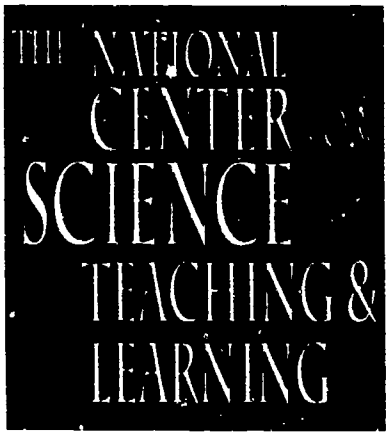

The NCSTL Monograph Series
Number 5

Rethinking the Form and Function of Scientific Research in Science Education

Robert Donmoyer

In this review and critique of traditional research in science education, Professor Robert Donmoyer of The Ohio State University argues that the tradtional mode of research is inadequate and inappropriate, and that the problems of "idiosyncracy and immaculate perception" will require dramatic change within the research community if research is to have a positive impact on science education practice.

THE BASIC PREMISE TO BE EXPLORED HERE CAN BE STATED SUCCINCTLY: the conception of educational research which has dominated our thinking in the past and continues to influence how we think and what we do today is inadequate and, to some extent, inappropriate. The paper is divided into four sections. Section 1 explicates the traditional conception of research in education and discusses the role research was traditionally expected to play in the field. Section 2 focuses on more contemporary views about the form and function of research in education; evidence of both growing skepticism about traditional views and continued acceptance of traditional notions is presented.

The third section focuses on problems with the traditional view of research and the traditional view of research's role in applied public policy fields such as education. Two problems are discussed: the problem of idiosyncracy and the problem of immaculate perception. These problems, it is argued are sufficiently serious to require more than modest, incremental adjustments in traditional ways of thinking about and traditional ways of doing research.

The final part of the paper focuses on implications of the two problems discussed in Section 3 for rethinking the form and function of educational research in general and for constructing a research agenda for The National Center for Science Teaching and Learning in particular.

Traditional Notions

DURING THE FIRST HALF OF THIS CENTURY PROGRESSIVE educators had two interrelated items on their

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reform agenda: (1) taking the schools out of politics and (2) establishing a cadre of professionals who they assumed would make educational decisions on the basis of research rather than political considerations. Implicit in the early progressives view of both professionalism and educational research was a social engineering metaphor which emphasized establishing research based standard operating procedures for practice and hierarchical control to insure these procedures got implemented. (Callahan, 1964; Tyack, 1972).

Often the social engineering metaphor was even made explicit. Franklin Bobbitt (1924), the father of the curriculum field, began his classic text, *How to Make a Curriculum*, by likening the work of curriculum developers to the work of engineers. Similarly, the father of the educational administration field, Ellwood P. Cubberly, wrote in 1909:

> Our schools are, in a sense, factories in which the raw products (children) are to be shaped and fashioned into products to meet the various demands of life. The specifications for manufacturing come from the demands for the twentieth-century civilization, and it is the business of the school to build its pupils according to the specifications laid down. This demands good tools, specialized machinery, continuous measurement of production to see if it is according to specifications, the elimination of waste in manufacture, and a large variety in the output (p. 338).

Raymond Callahan's (1964) historical account of the influence of the efficiency movement on school administration and historian David Tyack's (1974) description of school administrators' search for a "one best system" demonstrate that Cubberly and Bobbitts' social engineering orientation was shared by the field in general.

Researchers encouraged educators' social engineering view of professionalism and suggested that research could provide the necessary knowledge base to make social engineering possible. In 1910, for instance, Thorndike wrote in the lead article of the inaugural issue of *The Journal of Educational Psychology* that

> [a] complete science of psychology would tell every fact about everyone's intellect and character and behavior, would tell the cause of every change in human nature, would tell the result which every educational force—every act of every person that changed any other or the agent himself—would have. It would aid us to use human beings for the world's welfare with the same surety of the result that we now have when we use falling bodies or chemical elements. In proportion as we get such a science we shall become masters of our own souls as we are now masters of heat and light. Progress toward such a science is being made (p. 6).

John Dewey, of course, proposed a somewhat different, much less mechanistic version of social engineering. As a historian of American education recently noted, however, understanding the history of American education in the Twentieth Century begins with the realization that Dewey lost and Thorndike won.

Mechanistic visions of social engineering images did not leave public consciousness after early progressives such as Thorndike, Cubberly and Bobbitt were no longer players in the public arena. As late as the 1970's federal policymakers funded elaborate planned variation studies which policy analysts assured them would tell which policies and programs were most effective and consequently which should be mandated or funded. Within education, Project Follow Through (Abt Associates, 1977; Haney & Villaume, 1977) is the best known example of a planned variation study. The goal of this program was to determine the relative effectiveness of different early childhood education models in educating disadvantaged (the adjective of choice of the time) students.

Other indicators that social engineering imagery influenced thought and practice well into the second half of this century include the use of discrepancy models of program evaluation which assessed particular programs by comparing them to a model program which research established as

successful in another site; the majority of the research conducted in educational psychology including process-product studies of teaching, virtually the only research on teaching conducted prior to the 1970's (Good, Biddle, & Brophy, 1975); a host of programs and practices such as competency based teacher education and competency based

> "The question should not be, 'Do they deviate?,'... but rather, 'Are they adapting well to their respective environments?'"

teacher evaluation which were legitimated by either explicit or implicit references to process-product studies of teacher effectiveness; and the publication, in 1963, of Campbell and Stanley's *Experimental and Quasi-Experimental Designs for Research*, a book which codified Thorndike's vision of research and served (and continues to serve) as a rule book for many who play the research game.

Contemporary Thinking

OVER THE PAST FIFTEEN OR SO YEARS ONE CAN FIND evidence both of a growing skepticism about the traditional vision of educational research and of the resilience of traditional views. This section documents both trends.

INCREASED SKEPTICISM. Over time, members of the research community, at least, have became increasingly skeptical about researchers' ability to deliver the sort of knowledge base which would make social engineering possible. The inability of the Project Follow Through to provide definitive results, for instance, generated skepticism in the minds of many who previously had espoused the virtues of planned variation studies (Rivlin & Timpane, 1975). One group of scholars, after reviewing Project Follow Through data, took note of the probabalistic nature of the findings of the study and indicated that this aspect of the findings

> should be honored widely and serve as a basis of educational policy. Local schools do seem to make a difference. The peculiarities of individual teachers, schools, neighborhoods, and homes influence pupils' achievement far more than whatever is captured by labels such as basic skills or affective education (House, Glass, McLean, & Walker, 1978, p. 462).

Discrepancy evaluation models also have begun to be criticized. Spindler, for example, wrote these comments about a discrepancy-oriented evaluation of programs established by the Youth Employment Demonstration Act: "My first reaction was, 'Why would anyone expect different programs in different urban sites to replicate a model program in another site?' This expectation is against the first law of sociocultural systems in that all such systems (and a program of any kind is a sociocultural system) are adaptations to their environment. We should expect each program to show significant deviation from an initiating model, and from each of the other programs. The question should not be, 'Do they deviate?' or even 'How do they deviate?', but rather, 'Are they adapting well (functionally) to their respective environments?'" (Spindler, as cited in Fetterman, 1981, p. 70).

Furthermore, even Thorndike's field of education psychology has undergone some rather dramatic changes over the past several decades. By the mid 70's for instance, a new line of research on teaching—one which focused on the complex process of teacher thinking rather than discrete teaching behaviors (e.g., Clark & Yinger, 1977; Shulman & Lanier, 1977) had begun to be established. Even Gage, a die-hard supporter of process-product models of research on teaching, was forced to acknowledge in 1978 that such research could, at best, only provide a general knowledge base for teaching and that teacher artistry would always be required to adjust and shape that knowledge base to the needs of particular students and particular situations.

Today process-product studies are difficult to find within research on teaching literature. That literature is dominated by studies of teacher thinking, a subject which is normally investigated with methods more associated with the largely descriptive discipline of anthropology than with Thorndike's social engineering oriented field of study. Even single case studies, the sort of studies which Campbell and Stanley's rulebook indicated had "such a total absence of control as to be of almost no scientific value" (Campbell and Stanley, 1963, p. 6) have begun to be published by some of our most prestigious and selective journals, including *The Journal of Research in Science Teaching*.

VESTIGES OF THE PAST. Playing counterpoint to the evidence above is evidence which suggests that social engineering imagery is alive and well and very much present in the thinking of contemporary policymakers and even researchers. Evidence of the resilience of traditional modes of thinking is particularly plentiful in the areas of policymaking and program development

Teacher proof curricula such as DISTAR, for instance, are still being used in schools across the nation, especially in special education programs (Kuder, 1990). Also states such as Florida, Tennessee, and Texas have used the probabalistic findings of teacher effectiveness research to design checklist type instruments to assess teachers' competence and make certification and merit pay decision. Florida did abolish its merit pay system recently when some of the state's most dedicated and gifted teachers failed to score high enough on the state's effectiveness instrument to qualify for extra compensation, but elsewhere the practice of using fool-proof, research based systems to assess and reward teacher performance continues.

At the federal level, the latest buzzword is *systemic reform*. Both the National Science Foundation and the Office of Educational Research and Improvement have endorsed the concept and have attempted to reorganize at least some of their programs around it. It is too early to know precisely how this concept will play out in practice (indeed, at the moment different federal agencies seem to be attaching different meanings to the term), but the rhetoric suggests, fairly unequivocally, that social engineering imagery undergirds the notion of systemic reform. Furthermore, the rhetoric on systemic reform seems much closer to the Cubberly/Thorndike mechanistic version of social engineering than it does to the less mechanistic, less control oriented version propounded by Dewey.

There are, of course, some significant differences between earlier mechanistic views of social engineering and more contemporary systemic reform initiatives. The NSF version of systemic reform, for instance relies less on social science research as a basis for decision making and more on political coalition building. Also, most systemic reform models, at least, acknowledge the need for some discretionary decision making at the local level. (See, for example, Smith, 1991.)

Similar vestiges of earlier ways of thinking can be seen within the research community. Within the research community, however, vestiges of the past seem less a conscious recommitment to the notion of social engineering and more a somewhat thoughtless adherence to tradition and the standard operating procedures of the past. This phenomenon is no more evident than in the field of science education. Indeed, a reliance on tradition and traditional ways of doing and thinking about research is displayed quite clearly on the pages of *The Journal of Research in Science Teaching*, the premier research journal of that field within the United States.

Let me quickly acknowledge that *The Journal of Research in Science Teaching* has published some truly innovative work in the last few years. This work, however, remains on the periphery of the field. Evidence of this fact can be found in an editorial which appeared in a 1991 issue of the journal. In the editorial, the journal's editor, Ron Good, reprinted the guidelines which are sent to all reviewers of manuscripts which have been submitted to the journal. After acknowledging that the guidelines "have been used by previous *JRST* editors in nearly the same form, before qualitative research in science education became as prevalent

Figure 1: *JRST* Reviewer Guidelines

SOME GUIDELINES FOR REVIEW

The following items/questions are intended to assist you in evaluating and writing a review of the enclosed manuscript. This list is not necessarily exhaustive nor will each item apply to every type of manuscript. You will have to choose those criteria which apply to the manuscript enclosed and supplement the list as the need arises. You might wish to use your responses to the appropriate criteria in composing your review of the manuscript.

1. Title of Paper
a. Is it descriptive of the study?
b. Will it facilitate easy retrieval through search system?
c. Should the title be changed? If yes, what do you suggest?
d. Does it contain key words or phrases needed in information search systems?
2. Abstract
a. Is it specific enough to communicate the principal parts of the paper?
b. Is it succinct and accurate? If not, which parts should be removed or changed?
c. Is it missing any critical information? If so, what?
3. Introduction/Rationale
a. Does it discuss the importance of the study for science teaching?
b. Does it provide a link between the problem and the study design?
c. Does it establish a relationship between the study and previous work?
d. Is the rationale based on pertinent, essential work or vague generalizations?
e. Is the definition of the problem adequate?
f. Are the specific questions reasonable in light of previous research?
4. Method
a. Is the method justified?
b. Is the sample documented and properly selected?
c. Are the treatments specified in sufficient detail to allow for replication?
d. Are the models used documented and explained?
e. Are techniques for data collection appropriate to the enquiry? Have they been adequately specified?
f. Are data collection instruments valid and reliable? Are they justified?
g. Is the statistical power of the study discussed?
h. If the study tests hypotheses statistically, are the safeguards against error explained and defended?
5. Results
a. Is the data analysis appropriate to the question?
b. Is the data analysis sufficient? (Are means, standard deviations, sums of squares, degrees of freedom, explained variance—reported where appropriate?)
c. Are the data tables easy to read and complete?
d. Are the necessary data reported? If not, what is needed?
e. Are unnecessary data reported? If so, what should be deleted?
f. Are the illustrations appropriate/necessary?
g. Has an adequate description been given of the setting of the study and observations made to present a convincing case?
6. Interpretation
a. Are the conclusions appropriate to the findings of the study?
b. Are alternative interpretations recognized and discussed?
c. Are limitations to the study identified and discussed (low power, multiple significance, tests, etc.)?
d. Is the discussion congruent with the introduction/rationale for the study?
e. Are the implications for science teaching specified and explained?
f. Did the author(s) make an effort to translate theory into practice?
7. References
a. Is the reference list adequate for the study?
b. Are key references missing? If so, which ones?
c. Are the references outdated, inaccurate?
d. Are the references cited accurately used?
8. General Features
a. Is the paper easy to follow?
b. Are headings used appropriately?
c. Should specific sections be shortened/lengthened?
d. Is the writing style concise? Is the argument clear?

as it is today" (p. 291), Good presents a list of the present actual guidelines (Figure 1).

What is interesting here is not just that many of these criteria are quite inappropriate to assessing qualitative work (a problem Good acknowledges) or that it has taken so long to recognize this and begin to do something about it (One of Good's acknowledged purposes for printing the guidelines was to solicit suggestions about how to "make the guidelines more sensitive to both qualitative and quantitative research issues" [p. 291].). What is particularly interesting about the editorial is the total absence of even one hint that the utility and desirability of the guidelines listed and of the work produced by those who follow the guidelines may need to be reconsidered and critically examined. Rather what we see is a response which smacks of incrementalism. A few more people will be let in the tent; an alternative entrance might even be provided and a new wing might even be built. (This is the strategy employed by the American Educational Research Association. The *American Educational Research Journal* now has two distinctly different sections, each with its own editor, discourse style, and even reference procedures.) Fundamental questions about the form and function of the space we inhabit will be avoided, however.

Incrementalism, of course, is highly functional. Schema theorists, for example, note that it is normal for individuals to try to assimilate novelty into existing ways of thinking and acting and, when novelty cannot be assimilated, to try to accommodate novelty without totally upsetting one's conceptual applecart. These tendencies promote psychologized stability, virtue, to be sure.

Furthermore, stability is at least as much of a virtue at the organizational/sociological level as it is at the level of the individuals and individual psychology. There comes a time, however, when the problems are too great to be resolved incrementally. In the next section of this paper I will discuss two problems which I believe call out for more radical solutions.

Two Problems

I have applied labels to the two problems I want to discuss. One I call the *problem of idiosyncracy*; the other I have dubbed the *problem of immaculate perception.*

THE PROBLEM OF IDIOSYNCRACY. Let me try to illuminate the first problem by means of a story. The story is about two of my colleagues at Ohio State. Several years ago these two colleagues received a research grant to study young children's development. The two professors made a rather odd couple. One was an accomplished social scientist by training and temperament, a person well schooled in statistical analysis and research design. His co-investigator was equally bright, but she had spent most of her career working as a teacher. Their different backgrounds created creative tensions and many disagreements.

> The response: "'Teachers do not teach categories or types; they teach children.'"

One ongoing problem revolved around the teacher's complaint that none of the statistical descriptions they were generating described any of the actual students they had studied. The social scientist acknowledged this fact but could not see why his co-investigator considered this a problem. Social science research, he explained patiently (and eventually somewhat impatiently) focused on commonalities and generalities; it described types or categories of people not actual people. To this the teacher/teacher educator's response was always the same: Teachers do not teach categories or types; they teach children.

The teacher's comments get to the heart of the problem of idiosyncracy. The problem can be stat-

ed as follows: social science is about ideal types; the concern of a field like education, however, is ultimately individuals not aggregates.

The research community's growing realization of this problem and its significance can be seen in the career of the eminent educational psychologist, Lee Cronbach. By the mid 1950s Cronbach had already established himself as a skilled player of the Thorndike/Campbell and Stanley research game. In 1957, however, Cronbach told the American Psychological Association that the complexity of human phenomena requires a minor alteration in the traditional research game plan. Rather than searching for laws which were universal and context free, Cronbach argued, researchers should attempt to identify cause-effect relationships between certain educational treatments on the one hand and certain types of individuals (in Cronbach's terms, individuals with certain aptitudes) on the other.

In the mid 1970s, however, after nearly twenty years of searching for aptitude x treatment interactions and nearly twenty years of frustration brought on by "inconsistent findings coming from roughly similar inquiries," Cronbach (1975) told the American Psychological Association:

> Once we attend to interactions, we enter a hall of mirrors that extends to infinity. However far we carry our analysis—to third order or fifth order or any other—untested interactions of still higher order can be envisioned. (p. 119)

Compounding the problem of complexity was the problem of culture. Cronbach cited Bronfenbrenner's historical look at child rearing practices of middle- and lower-class parents. Class differences documented in the 1950s were often just the reverse of practices that had been observed in the 1930s. Cronbach concluded:

> The trouble, as I see it, is that we cannot store up generalizations and constructs for ultimate assembly into a network. It is as if we needed a gross of dry cells to power an engine and could only make one a month. The energy would leak out of the first cells before we had half the battery completed. So it is with the potency of our generalizations (p.123).

In his 1975 article, Cronbach emphasized that the social world was no less lawful than the physical world. The problem was that social laws were too complex and the social world too changeable to identify them. By the early 1980s, however, Cronbach had rejected even the notion of social laws. He began suggesting that the entire cause-effect way of thinking which undergirds the traditional view of research is an inappropriate way to characterize social phenomena. By 1982, in fact, Cronbach had arrived at a position similar to that of symbolic interactionists (Blumer, 1969) and ethnomethodologists (Garfinkle, 1967): Human action is constructed not caused; those who expect research to produce the sort of definitive cause-effect generalizations promised by Thorndike are simply, in Cronbach's words, "Waiting for Godot."

Not everyone has arrived at as radical a position as Cronbach's, of course. Phillips (1987), for instance, has argued that Cronbach has underestimated the complexity of physical phenomena and, hence, overestimated the relative complexity of phenomena in the social world. Others might point out that researchers are quite capable of generating probabalistic findings which can, at least, inform us of the likelihood that a particular educational treatment will produce a particular educational outcome. Few, however, would dispute the fact that even if Phillips is correct when he argues that the social world is no more complex than the physical world (a questionable assumption given the fact that Phillips does not even address the changeability of culture issue raised by Cronbach), educational purposes are almost always more complex. An engineer employing the theory of quantum mechanics, for example, is not interested in what happens to individual electrons; probabalistic findings, therefore, will be more than adequate to accomplish the engineer's purpose. Teachers, however, do care about individual students; probabalistic findings, therefore, have limited utility in accomplishing complex educational purposes. A probabalistic finding, after all, not only tells us

what is likely to occur; it also reminds us that what is unlikely may very well happen.

In short, no matter how large our sample, we can never know whether research findings will apply to a new and different case or even to a particular case in the original sample. This fact certainly does not require that we totally reject the traditional research game. It does require, however, that we reject Thorndike's notion that social science research can provide the sort of knowledge base which could support social engineering.

THE PROBLEM OF IMMACULATE PERCEPTION. The problem of immaculate perception has been recognized in an array of disciplines and fields of study, although each discipline and field tends to characterize the problem in a somewhat different way. Philosophers of science talk in terms of paradigms or conceptual frameworks. Psychologists use the terms *schema* or *cognitive structures*. Anthropologists talk of cultural constructs; literary critics of interpretive frameworks. I am not suggesting that each of these terms is a precise synonym for the other; each, however, alludes to the fact that we do not have direct access to reality and that our perceptions of the empirical world will always be influenced by (often unconscious) a priori conceptions of what reality is and ought to be.

The significance of this problem for a field like education can be demonstrated by considering a term such as *learning*. Few people would disagree with the proposition that schools should promote learning, but the term *learning* will mean quite different things to different people, to Piagetian and Skinnerian psychologists, for instance. Before a researcher can determine whether Program A produces more learning than Program B, the researcher must choose one of the paradigms—i.e., one of the meanings—alluded to above or one of the multitude of other meanings which could be associated with the term *learning*. The meaning selected will influence the researcher's findings at least as much as the empirical reality being described.

The situation is further complicated by the fact that, from certain paradigmatic perspectives, the whole cause-effect way of thinking and talking employed by traditional researchers becomes problematic. Freire (1970), Buber (1968), peace educators like Galtung (1974), and a humanist reading of Dewey (see Kleibard 1975), for example, suggest that educational practice should not be built around predetermined student learning outcomes, no matter what conception of learning the predetermined outcomes reflect. This position suggests that rather than attempting to control students, teachers should engage in dialogue with students, and rather than transmitting a predefined curriculum to students, teachers should work with students to construct jointly the curriculum for the class.

These prescriptive educational theories are compatible with the theoretical descriptions of human action put forth by symbolic interactionists, ethnomethodologists, and the 1980's version of Cronbach. Whether one agrees with these descriptive theories or not, they do provide an alternative to Campbell and Stanley's conception of how the social world operates. As such, they remind us that Campbell and Stanley's cause and effect conception of the social world is just that, an a priori conception, a conception which is not determined by the facts but rather determines what the facts are. In short, they reinforce a point made by Kant years ago: It is impossible to talk of the nature of reality with any sense of certainty because we can never know reality independent of the cognitive structures which influence our perceptions.

Implications

In this final section of the paper I want to briefly consider some implications of the two problems outlined above. General implications for rethinking the form and function of science will be discussed. I will also provide more specific examples of how the problems outlined above have influenced the development of the National Center for Science Teaching and Learning's research agenda.

IMPLICATIONS OF THE IDIOSYNCRACY PROBLEM. One obvious implication of the problem of idiosyncracy is the realization that research will never be able

to provide prescriptions for practice; at best it will serve only a heuristic function. Of course, this does not mean that research is useless. Weiss (1982), for example, suggests one heuristic function for social science research in her study of policy makers' utilization of research in formulating health policy. She indicates that research was of little use in problem solving but very important in problem framing. Social science research helped structure policy makers' thinking by supplying language with which to conceptualize policy questions. In the process, research directed policy makers attention to possibilities and options which probably would not have been considered in the absence of research. Social science research can certainly serve a similar role in the decision making of teachers and other educational professionals.

Social science research can also tell us what is typical and give us some sense of what will typically occur if we employ different types of educational strategies. This sort of information will be especially useful to educational policymakers. Unlike teachers who must be concerned with educating idiosyncratic individuals, policy makers are primarily concerned with aggregates. Like social scientist, it is functional for policymakers to think in terms of *types* of people.

Policymakers, of course, must realize that if educators are to meet the needs of students they must design policies and programs which allow for considerable discretion at the school and classroom levels. After all, a probabalistic generalization which tells us what will typically occur also informs us that the atypical will occur with some individuals in some settings. Therefore even relatively definitive research findings do not automatically translate into policies and programs. Once again, they serve only as a heuristic, not as a recipe.

The heuristic nature of research certainly suggests the need to reconsider standard operating ways of thinking about research and standard operating ways of doing it. For instance, given Weiss' conclusion that research helps us to frame rather than to solve problems, it might make sense to think of the whole matter of generalization in psychological rather than in statistical and sampling terms. If we do this, single case studies suddenly appear to have far more utility than was traditionally thought. (For an in-depth exploration of this idea, see Donmoyer, 1990.)

At the very least, an understanding of the problem of idiosyncracy forces us to lower our expectations for large scale research projects. At best such projects will net findings which will have to be shaped and adjusted by actors at the local level to fit local contextual variables. This recognition of the heuristic function of research and the importance of the local may lead to a blurring of the traditional distinction between research and development efforts, such as the work carried out by Berlir and White at the NCSTL (1992).

IMPLICATIONS OF THE IMMACULATE PERCEPTION PROBLEM. Just as the idiosyncracy problem calls into question traditional ideas about generalization, the problem of immaculate perception challenges traditional notions of objectivity. Furthermore, the recognition that the frames people bring to a problem to a large extent dictate the solutions they find also has influenced the Center's decision to bring together diverse constituencies (e.g. science teachers, teacher educators, administrators, practicing scientists from multiple disciplines, business persons, and policymakers) which employ very different frames of reference to discuss and analyze the problems of science education. The purpose in doing this is a bit different from the purpose which appears to undergird NSF's coalition building activities in the systematic reform initiatives it has attempted to promote. We see such gatherings as a form of inquiry, a form of inquiry which hearkens back to Aristotelian notions of deliberation and practical reasoning. (For further discussion of these ideas, see Donmoyer, 1991a, 1991b, 1990.)

One additional implication of the problem of immaculate perception involves the need to expand the substantive focus of our research. We need to focus on how teachers and others conceptualize science and how different cultural perceptions can affect the learning of science as it is conceptualized

within the culture of schools. To adequately investigate cultural phenomena, we may also need to employ methods from fields such as anthropology. Such methods are now widely accepted within the educational research community in general but are less commonplace form in the subfield of science education.

Summary

IN THIS PAPER I REVIEWED TRADITIONAL NOTIONS about the form and function of educational research, examined contemporary manifestations of these notions, and discussed two problems with traditional ways of conceptualizing what research is and what role research should play. I also briefly discussed some implications of these two problems for reconceptualizing the form and function of educational research in general and formulating a research agenda for the National Center of Science Teaching and Learning in particular.

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